Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Mark T Stacey

Final Selection Panel Review

Proposal Title

#0136: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Funding:

Fund with future funds
Amount: **$967,525**

The final Selection Panel agreed with its original recommendation on the merits of this proposal. Due to the recent reduction in funds available for the Science Program's 2004 PSP, the Selection Panel has been forced to place this proposal in the Fund with Future Funds category. This decision was based solely on the current programmatic priorities of CALFED and the current level of available funds for purposes of supporting research efforts of this nature. This decision was not a reflection of the technical merit of this proposal.

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0136: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Funding:

Fund
Amount: **$967,525**

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

The proposal specifically addresses the fluid mechanical processes controlling the distribution and redistribution of sediments in San Pablo Bay over a range of temporal and spatial scales. Habitat niches in this embayment are clearly a function of sediment distribution and fluxes (and contamination), especially given the strong channel−shoal structure of the Bay. Factors include relative light penetration, bathymetry, substrate stability, and contamination. The proposers present a plausible argument that wetland restoration efforts along the northern edge of the Bay will be in a number of ways dependent on the nature of sediment movement through the Bay, establishing a connection between the proposed work and the implications of future changes in land use surrounding the Bay on environmental

processes. Because of the presence of contemporary and legacy contaminants in the sediments of the Bay−Delta system, the proposed work also has relevance to the general water quality goals of CALFED. A number of contaminants with significant ecosystem and human impact are strongly associated with sediment particles, and their mobility in the environment is largely mediated by sediment movement. The proposed work also has specific relevance to CALFED's broad ecosystem restoration goals, especially in portions of the Bay−Delta system being returned to tidal action after having tidal mixing suppressed or eliminated by dikes, etc. Because of its grounding in fundamental fluid mechanics observation and analysis, the proposed work has quite a bit of generality, both for understanding other embayments in the Bay−Delta system with channel−shoal bathymetry, and for understanding sediment distribution and flux in estuarine environments in general.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

Based on the reviews, the budget appears appropriate and proportional. None of the reviewers nor the Technical Synthesis Panel provides any suggestions for budget streamlining or identifies any nonessential components of the study, and I do not have the expertise to identify any such opportunities myself.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

It is very difficult to provide an a priori rating of this sort at this stage of the process. Based on its relevance, the reviews, and the Technical Synthesis Panel evaluation (Excellent), I think the proposal is worthy of funding.

Selection Panel (Discussion) Review

fund this amount: $**967,525**
note:
fund

This proposal focuses on understanding the movement of sediment in San Pablo Bay a bay dominated by channel−shoal interactions. It is relevant to two PSP study topics: environmental influences on key species and ecosystems and implications of future change on regional hydrology, water operations, and environmental processes. It is particularly relevant to the design of wetland restoration along bay shorelines by contributing to understanding of relative sources of sediment, sediment accumulation and erosion, and potential contamination via sediment−bound contaminants such as mercury. The technical reviews were very positive. The synthesis panel argued that critiques that were not excellent were based on future next steps, rather than weaknesses in the work proposed here. The panel felt that the proposal is technically excellent, with a great group and a high likelihood of success on their proposed research.

However, the panel felt that, in spite of being a doable project, an excellent team, and a solid proposal, it may have too narrow a strategic focus. For example: vegetation effects are not incorporated. And it is not described how water operations and potential changes in operations could be affecting hydrodynamics and sediment transport in this setting. This proposal should be considered for funding, but the panel expressed reservations about the lack of broad strategic implications of the work.

Panel Ranking: Fund

Technical Synthesis Panel Review

Proposal Title

#0136: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Final Panel Rating
superior

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The goal of this proposal is to improve understanding of how sediment is redistributed in San Pablo Bay, including details on how exchange occurs between the channels and shoals. The approach includes analysis of historical data sets, broad observations of circulation in San Pablo Bay, and sediment coring. The work intends to integrate hydrodynamic, sediment transport and chemical tracer measurements over daily, springneap, wintersummer, and decadal time scales. Application includes improved evaluation of sediment quantity and quality for wetland restoration, with immediate application to the northern edge of the Bay, and to the transport and fate of contaminated sediments. There is a direct connection to marsh restoration. "If restored wetlands draw sediments from existing San Pablo Bay sediments, then the wetlands will be high in mercury and will likely export methyl mercury to the surrounding region. Alternatively, if the sediments that provide most of the accretion are new sediments from the surrounding watersheds, there will be less risk of mercury methylation." The proposal received three reviews, with ratings EXCELLENT, VERY GOOD, AND GOOD. All reviewers found the explanation of the objectives and the supporting conceptual model to be very good. The reviewers generally found the work to be ambitious, but with a clearly laid−out

set of tasks. Two reviewers commented that the connection between the actual observations and habitat restoration on the north edge of the Bay could be clearer. One mentioned that the role of extreme events should be considered more thoroughly. Another would have preferred placing the work within the context of longer term monitoring and a large scale numerical model. These comments should be helpful in evaluating the project design. A rating of SUPERIOR is based on a number of factors. The proposal provides a clear and strong statement of the applied relevance of the work—marsh restoration requires an improved understanding of the quantity and quality of sediment supply—and the basic research relevance—we need to better understand sediment cycling in estuaries. An excellent conceptual model is presented describing the investigator's understanding and expectations. This is distilled into clearly stated and relevant research questions. The methods (analysis of historic data, water and sediment flux measurements, sediment coring) are well explained, appropriate, and clearly connected to the research questions. Accounting for the effect of extreme events and placing the work within a longer−term and larger scale monitoring and modeling program would be logical extensions of the proposed work, but is not essential to have confidence in the high likelihood of useful results.

Additional Comments:

The goal of this proposal is to improve understanding of how sediment is redistributed in San Pablo Bay, including details on how exchange occurs between the channels and shoals. The approach includes analysis of historical data sets, broad observations of circulation in San Pablo Bay, and sediment coring. The work intends to integrate hydrodynamic, sediment transport and chemical tracer measurements over daily, springneap, wintersummer, and decadal time scales. Application includes improved evaluation of sediment quantity and quality for wetland restoration, with immediate application to the northern edge of the Bay, and to the transport and fate of contaminated sediments. There is a direct connection to marsh restoration. "If restored wetlands draw sediments from existing San Pablo Bay sediments, then the wetlands will be

high in mercury and will likely export methyl mercury to the surrounding region. Alternatively, if the sediments that provide most of the accretion are new sediments from the surrounding watersheds, there will be less risk of mercury methylation." The proposal received three reviews, with ratings EXCELLENT, VERY GOOD, AND GOOD. All reviewers found the explanation of the objectives and the supporting conceptual model to be very good. The reviewers generally found the work to be ambitious, but with a clearly laid−out set of tasks. Two reviewers commented that the connection between the actual observations and habitat restoration on the north edge of the Bay could be clearer. One mentioned that the role of extreme events should be considered more thoroughly. Another would have preferred placing the work within the context of longer term monitoring and a large scale numerical model. These comments should be helpful in evaluating the project design. A rating of SUPERIOR is based on a number of factors. The proposal provides a clear and strong statement of the applied relevance of the work—marsh restoration requires an improved understanding of the quantity and quality of sediment supply—and the basic research relevance—we need to better understand sediment cycling in estuaries. An excellent conceptual model is presented describing the investigator's understanding and expectations. This is distilled into clearly stated and relevant research questions. The methods (analysis of historic data, water and sediment flux measurements, sediment coring) are well explained, appropriate, and clearly connected to the research questions. Accounting for the effect of extreme events and placing the work within a longer−term and larger scale monitoring and modeling program would be logical extensions of the proposed work, but is not essential to have confidence in the high likelihood of useful results.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

This proposal was very well written. Basic and applied research goals were clearly stated and timely. The conceptual model was well−described and well−documented, resulting in a very strong scientific proposal. The panel noted that the

temporal scale of the proposal (short−term) may be inadequate to make accurate long−term predictions. Despite this concern (which the panel felt was unavoidable given the funding period of CBDA and other grants), panelists believed this project would produce results of high value on a topic of critical concern to CBDA.

Technical Review #1

proposal title: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Yes. The authors brought an interesting and important topic to address the cycling of sediments, its legacy and dispersal in San Pablo Bay. The proposed work will address one of the fundamental mechanisms of how a shoal−channel system works and its implications to the quality of habitat restoration in a Bay. The goal of the proposed work, its hypothesis and conceptual models are clear, timely, important and they are indirectly related to the CALFED Science program objectives.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The proposed work will enhance our existing knowledge of hydrodynamics and legacy sediment transport in San Pablo Bay. The conceptual model is well described and methodology is based on our existing knowledge of hydrodynamics and sediment transport mechanics in a shoal−channel system. The proposed research topic demonstrates the importance of sediment cycling in San Pablo Bay and the outcome can be used in similar**

	Bay−Delta system. In this aspect, it can be termed as a DEMONSTRATION project. The research methodology, however, lacks to address (clarify) one of the important links between sediment cycling and habitant restoration and its feedback to the overall Bay ecology.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**As mentioned earlier, the proposed work indirectly relates to the objectives of the CALFEED Science program. The approach and methodology are well designed. The results will be helpful in understanding the basics of hydrodynamics and sediment transport mechanisms in San Pablo Bay and other similar bays. The information will also be helpful for the decision makers considerably.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Yes, the approach and conceptual models were well documented. Authors have relevant expertise to handle such cases. I also think that authors are capable to complete the project within the proposed time frame.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Not Applicable. Because of the type of the work, the proposed project does not have any monitoring plan.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The product from the proposed research work will create a knowledge base for San Pablo Bay in terms of sediment transport mechanisms and their cycling which will help the scientific community and planners to identify some of the fundamental questions of the overall ecology of the Bay.**
Rating	**good**

Additional Comments

Comments	**Although it is understood that there is a crucial link between sediment cycling and the ecological health of a bay, it is not quite clear how the researchers will address this issue (Link between habitat restoration and sediment transport &hydrodynamics). In that sense, the proposed work indirectly related to the CALFEED Science mission especially in the question of habitat restoration. In addition to that the research project did not elaborate methodology for handling sediment transport and re−suspension under extreme meteorological events which is important in balancing the equilibrium of any Bay−Delta system.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors are highly compatible in carrying out such projects. The investigators are reputed in this field and have enough resources to carry out such projects efficiently and ina timely manner.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Yes**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I think the project is interesting and will enhance our understanding of Channel−shoal system. However, the project needs to address and focus one of the key issues of linking between sediment transport and how this information can be related to habitat restoration and overall quality of the Bay. Also realizing the importance of extreme meteorological events in the morphological balance of any Bay−Delta system, the proposed work should device some kind insights to address this issue.**
Rating	**good**

Technical Review #2

proposal title: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The overall goal of this proposal is to assess general circulation patterns, sediment transport rates and pathways, sediment inputs and outflows from up−estuary and down−estuary, and the controlling processes in San Pablo Bay. The investigators intend to use a very comprehensive field program consisting of state−of−the−art equipment such as ADCP current meters, pressure sensors, optical backscatter sensors, salinity meters, etc. The project will also include extensive sediment sampling and geochemical analyses. Finally, previous work such as bathymetric surveys and tracer studies will be utilized. Overall, this is a very ambitious project using a holistic approach that is very convincing. The proposal clearly states the goals, objectives, and hypotheses and is internally consistent. The proposal also demonstrated the timeliness of this work.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is well justified in respect to existing**

	understandings of sediment dynamics in estuaries and in San Francisco Bay. The proposal addresses the use of the results towards managing tidal marsh reclamation in the regions, as well as potential issues with mercury or other contaminants of concern being resuspended, transported, and deposited in reclamation areas. I find the arguments convincing. The conceptual models being tested is clearly laid out.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach being used in this study is to integrate: historical data (bathymetry and an iridium tracer studies in San Pablo Bay) to assess previous bathymetric changes and sediment transport pathways; new sediment studies using cores and radionuclides to determine recent sedimentation and mixing rates and processes; and hydrodynamic and suspended sediment dynamics studies to determine circulation and sediment transport pathways and rates. The field programs are designed to look at processes on time scales from hours to seasonal. The approach being used is well described and will meet the objectives of the study. The work is also feasible and the investigators are well experienced in the field deployments of the equipment. In addition, the USGS is providing an impressive array of equipment for the project.** **I think the results will advance our understandings of estuarine sediment dynamics and will provide information very useful to managers in the region.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As explained previously the approach is feasible and should be successful. The scale of the project should provide the temporal and spatial observations that are needed. The techniques and instruments being used are proven and the investigators are experiences with these types of studies (based on the proposal and the publications).**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Not applicable.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The primary outlet for the results of this work will largely be scientific publications in peer−reviewed journals and presentations at scientific meetings including the CALFED science conference. Publications and presentations are appropriate outlets for disseminating the results of this study to the scientific community and managers. In addition, the interactions of the USGS with the public should also make the results of this research readily assessable to local groups and managers.**

Rating	**excellent**

Additional Comments

Comments	**None.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Based on the proposal, the references cited and the CVs, the investigators appear highly qualified, publish their results in respected journals, and complete projects. In addition, the necessary infrastructure is available to accomplish the project.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**It is difficult to judge the budget for this study, but the total costs seem to be in line with the magnitude and length of the study.**
Rating	**not applicable**

Overall

Provide a brief explanation of your summary rating.

Comments	**In general, I found this proposal to be very well written, very comprehensive, and addressed all the relevant criteria needed t explain the rationale, the approach, and the need for the work. I found very few weaknesses and felt the investigators' arguments were**

	convincing. I could not judge a couple the physical oceanographic concepts, as I do not have the appropriate background in hydrodynamics. However, most of the explanations were largely conceptual and easy to follow. Furthermore, the sedimentological aspects were fundamentally sound.
Rating	**excellent**

Technical Review #3

proposal title: Hydrodynamics and Sediment Transport in a Shoal−Channel Estuary: The Cycling of Sediments in San Pablo Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**A very large number of hypotheses are presented. They seem logical and well thought−out, but I do not think it will be possible to address all of them through the research described in the proposal. One fundamental issue − that a knowledge of sediment transport and fate is important for contaminant transport considerations − remains timely, and important. The importance of the chosen study area over other candidate sites was less clear to me. It seemed like a major reason for choosing the site was that it had received less attention in the past. Which is a valid reason in some situations.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Many conceptual models are stated. Some have been tested at other sites and would be tested at a new site through this research. I do believe that knowledge would be gained about San Pablo Bay circulation and sediment transport, and that some of this could lead to ideas that are applicable at other**

	sites. Two of the problems mentioned as motivation or justification are contaminant transport and habitat restoration. The former would be addressed, for sorbed contaminants at least, via the focus on sediment transport; it was not clear to me how the latter would be addressed in any kind of direct way.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**I would say that the proposed project is very ambitious, in that a large number of objectives are stated. Regarding approach, there were several issues which raised my curiosity: 1) the time scale of interest was not completely clear to me. A decadal time scale would seem to be of major importance, although I can see the rationale for resolving other time scales, such as seasonal. But the field campaigns (2−6 weeks in several cases) that are described do not seem to be well−suited for longer time scales (years). 2) I had a similar concern regarding spatial resolution. For example, the importance of bedforms on transport is discussed, and the existence of bedforms will be documented. But the domain considered is quite large, and the number of measurements limited, both in space and time − what assumptions will be made about the regions where measurements are not available? 3) Regarding the historical data analysis: there was no mention of data quality (particularly vertical accuracy and resolution) and how this compares to expected changes. Will the signal to noise ratio for the period considered be sufficient to draw conclusions regarding long−term changes? The utility of the project results to decision makers was not emphasized in the proposal. I think that the project**

	would provide some useful findings for the decision makers, but that additional work would be required to address particular applied problems.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	My comments above address feasibility to some degree. I think that the work described is generally well thought out and the investigators well qualified, but that the project is too ambitious. So I think that it would answer some but not all of the questions posed.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	See my comments above regarding temporal and spatial resolution of the measurement campaigns.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The primary products that are mentioned are peer−reviewed publications and conference talks/papers. Some of the project results thus

	described could be of use for making management decisions; however the project is intended primarily to yield a validated description of processes under existing conditions. Prediction of the impacts of management changes would require further study or validation.
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**I did not have any concerns in this department. The team seems well qualified.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I found that the organization of the budget pages made them extremely hard to decipher (not the author's fault, presumably). It was difficult to see the sequencing of the different tasks (I could make a similar comment about the list of tasks), and it was difficult to see how much funding was being reserved for each person or when.** **So I did not pick through every item in the budget. Most expenditures seemed reasonable. One thing did catch my eye, however: 36 months, and $170k to compare two existing bathymetric surveys for volumetric changes. This seems extremely high; if I understand**

	the task, it seems that one graduate student could easily complete the job in much less than a year.
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposal describes many interesting hypotheses that would be very challenging to validate via field experiments. They seem to focus more on basic research questions than applied, and I felt that in general the proposal is a bit too optimistic, particularly by using multiple short−term field campaigns when long−term (years−decades) changes would seem to be of greatest interest. Given the scope of the problem, it seems that an approach that combines field measurements with large−scale numerical modeling might be more appropriate.**
Rating	**very good**